Reviewer's report

Title: Increased cancer risk in type 2 diabetes mellitus: results from a cohort study in Tyrol/Austria

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Reviewer: Matteo BONZINI

Reviewer's report:

I was pleased to revise the paper by Oberaigner and colleagues addressing the association between Type 2 diabetes and cancer incidence.

Major revisions

A have some issue that, if properly addressed or discussed, in my opinion would sensibly increase the paper. In fact manuscript, even if methodologically appropriate, is quite lacking in term of clarification of how it contribute to the already established knowledge and in term of discussion of the effect of possible bias.

1- As authors stated, several meta-analysis investigated and quantified the association between diabetes and cancer incidence. I think that authors should better address the following questions:

why we need another study? And why in Tyrol. The reason “because it is the first in central Europe”, seems too poor. Is Tyrol in same way characterized by prevalence ratio different from others? Or are Tyrolean registers better or different from other larger registers? And why authors stated Tyrol is representative of the Central Europe. We could affirm that, as a small mountain region, its population could have characteristics difficult to be reproducible in other similar setting.

2- There are too few info about the standard population. Only in abstract we read Tyrolean population served as standard. Being that, a key issue for the reliability of SIR, more info should be given.

3- The most of the increased risk are temporarily very close to diabetes diagnosis. Even if authors correctly stated “causality of the association is far from being the aim of our study”, all discussion and conclusion tends to interpreter association as “risk”. And it is natural doing that. But more attention should be paid to discuss the possibility that recent diagnosis of diabetes make people more carefully investigated (and prevalent cancers could be diagnosed: ascertainment bias), or that type 2 diabetes should be a symptoms of a pre-existent cancer (ex: pancreatic cancer).

4- As a study based on register comparison, we read that cancer registers has been validated and used before, but we have few info about diabetes register. It is important to discuss the impact of the fact that patients treated in private practice (that might be not so few) are not included in the study.
5- The paper is focused on incidence. This is not always clear across the text, for example when authors describe previous meta-analysis. I suggest to put incidence even in the title: “increased cancer incidence in type 2 diabetes mellitus:…”

Minor issues:
1- The manuscript is generally well-written, concise and clear, but there are some minor point in which English should be checked. For example:
- background line 11 relation(ship) between;
- line 14: “remain (the) matter of debate” or
- following page line 6 “we aimed (at) assessing”;
- results, fourth line: retrospectively used instead of respectively?
- discussion second page line 15 “a risk estimate of 2.10 (as) compared to a risk estimate of…”

2- Tables: please use an abbreviation for” Haematological and lymphatic system tumors”

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I can declare that I have no competing interests